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their success was too decided and therefore their amplitude too small; and that on their second determination they should have made it between 3″ and 4″ larger than at first. Svanberg, I consider, was sent to Lapland with a strong expectation that he would obtain a less value of a degree of the meridian than that of Maupertuis. See Melanderhjelm's preface to Svanberg's book."

Mr. Todhunter makes the following remarks on a question of mathematical history:—

"Svanberg alludes to the researches of Huygens on centrifugal force; and then he says that if we suppose with Huygens that the force of gravity resides in the centre of the Earth we obtain for the ellipticity $\frac{1}{578 \cdot 73}$. Then he proceeds with the strange statement that these discoveries were the precursors of the hypothesis which Newton afterwards proposed of universal gravitation. Thus Svanberg ascribes to Huygens and not to Newton the priority in investigations of the figure of the Earth. Now the first edition of the *Principia* was published in 1686. Huygens published in 1690 his *Discours de la cause de la Pesanteur.* Although it appears from the preface that some part of this work had been long before communicated to the Academy at Paris, yet Huygens excepts the part which relates to the motion of pendulums as affected by the figure of the Earth: and all that relates to the calculation of the figure of the Earth he expressly says was added after the publication of Newton's *Principia.* Perhaps Svanberg was misled by a hasty glance at Bailly's *Histoire de l'Astronomie Moderne,* vol. iii. p. 9. Bailly accidentally notices Huygens before Newton. Again Svanberg says, 'Newton ne tarda pas à remarquer, que l'hypothèse de Huygens d'une force unique tendante au centre de la terre, étoit équivalente à celle d'y supposer une densité infinie.' I do not know what this means. Huygens really used the hypothesis of a force constant in magnitude, and directed always to the centre of the Earth; it is of course only as to the latter part of the hypothesis that we can assert the equivalence of an infinite density at the centre. I do not think that Newton drew attention to this equivalence: Clairaut did in the *Philosophical Transactions,* vol. xl. pp. 277–306. Clairaut, I think, was the first who showed that Huygens' value of the ellipticity follows approximately from the latter part of Huygens' hypothesis, namely, that the force always passes through the centre, without assuming any particular law of intensity of the force at different distances. See Clairaut's *Figure de la Terre,* p. 141. See also Plana, *Astronomische Nachrichten,* No. 839."

The Orbit of Castor. By J. M. Wilson, Esq.

In the last number of the *Monthly Notices* I stated that the result of an examination of the orbit of *Castor* by graphical

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methods was that it was hyperbolic. These methods are rough, and in this case present peculiar difficulties. Since writing that note, I have attacked the question analytically, following the method of Sir John Herschel, in *Mem. Ast. Soc.* Vol. xviii., and have the pleasure to acknowledge much help in the arithmetical part of the work from my friend Mr. Henry Stevenson, of Bedford, who undertook the laborious task of solving the equations.

The method is as follows: The polar co-ordinates, r, θ, of the determined points are converted into rectangular co-ordinates, x, y. An equation is then assumed to a conic section, which passes through these points,

$$0 = 1 + \alpha x + \beta y + \gamma x^2 + \delta x y + \varepsilon y^2,$$

and by substituting the values of x, y for the points determined, as many equations are found for α, β, γ, δ and ε as there are points. These are combined by the method of least squares, so as to give the most probable result. I omitted the point corresponding to 1740·46 as resting on very insufficient evidence; and I am inclined to believe that a still better result would be obtained if the next two points were also omitted, or had much less weight assigned them.

By a singular coincidence, in the solution of the equation, the coefficients of x^2 and y^2 come out so small as not to affect the first seven places of decimals, and hence the curve is seen at once to be approximately a rectangular hyperbola, whose asymptotes are parallel to the axes, and whose eccentricity is 1·414.

The resulting equation is

$$0 = 1 + \cdot 008876 x + \cdot 008098 y + \cdot 0000433 x y,$$

A being the origin and the positive parts of the axes of x and y, being in the directions 90° and 180° measured in the usual manner from the north.

The eccentricity of the real orbit may hence be approximately computed at 1·625.

The divergence of this result from that obtained by graphical construction is at first sight very great, for by that the eccentricity of the apparent orbit was more than 2. But the graphical solution is not to be trusted, and in this case it is easy to make errors that prejudicially affect the eccentricity. One of my pupils—Mr. D. C. Baynes—made the curve nearly parabolic on one occasion, and on another he made the eccentricity 2·01. An error of a tenth or twentieth of an inch in measurement, or a corresponding error in drawing the hyperbola, would vitiate the result completely. To determine the centre and axes of an incorrectly drawn hyperbolic arc in the neighbourhood of the vertex is a very unsatisfactory problem. The analytically obtained result has more weight: that is, it has the weight, to the degree of approximation to which it has been carried, of that combination of the original observations which is given by the interpolating curve, smoothed by the method of differences to the third order, with no errors of drawing superadded.

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I have here a chart* showing all the observations that have been sent me, from which it can be judged how far the interpolating curve adopted is a fair representation of them; and a roughly drawn chart marking the positions of the stars at different dates, and the supposed hyperbolic arc on which they lie. The deviation of the curve from the early positions is great, but they rest on very few observations, as is seen from the other chart, and several of the early observations, as of the late ones, are many degrees in error.

It occurred to Mr. Seabroke and myself that it would be worth while to examine the spectra of A and B, to see whether the indication derived from the orbit, that they belonged to different systems, was confirmed; but the long delay of an optician in preparing part of the necessary apparatus has frustrated us for the present.

There appears to be something peculiar about the motion of this pair. Every successive determination of the eccentricity of the relative orbit has increased that eccentricity, as shown by Mr. Proctor in his note on my former paper. It may be thought worth while, by some one with more skill or more leisure than I possess, to subject the orbit to a searching examination, and this is my object in bringing the matter in a confessedly unfinished state before the Society.

My own conclusion is, that it is now probable that the orbit is really hyperbolic.† If so, *Castor* must be looked on, not as a binary system, but as two stars, whose relative proper motions have been in nearly opposite directions, and whose near approach has been witnessed once, and will never be witnessed again.

Temple Observatory,
Rugby, June 14, 1872.

On the Desirability of Watching for the November Meteors in the present year. By Rich. A. Proctor, B.A. (Cambridge.)

On the night of November 13–14, 1871, no members of the November meteor system were observed in England; and the conclusion was arrived at that the richer portion of the system has passed so completely beyond the Earth's orbit that no displays may be expected until the close of the century. Mr. Glaisher, in fact, promised his meteor-observing assistants at Greenwich that they should no longer be kept on the watch after midnight on November 13–14. There are reasons for believing, however, that the conclusion on which this promise was based is incorrect. What appears to have happened is simply this, that a more expanded portion of the meteor stream is now traversing the Earth's orbit, and that accordingly the passage of the Earth through the system

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* The publication of this chart is unavoidably delayed.

† My friend and former pupil, Mr. A. M. Worthington, of Trinity College, Oxford, has worked the whole question over again from the original data, and makes the orbit a hyperbola of small eccentricity, *i e.* nearly parabolic.